

beginners we should expect to be confined to the very simplest deductions, and these should be most carefully graduated, whereas the exercises before us do not seem to be arranged in any very clearly-defined order. For instance, at one time we are in the first book, in the next question in the sixth book, and then back to the third, and so on. The references are apparently to Euclid, but not to editions in use in this country, as our author inserts on pp. 118, 120, 121, propositions which figure as Euc. vi. B, C, D, in Simson's text; on p. 126 a solution is given from Lardiner's (*sic*) Euclid.

The solution on p. 112 strikes us as not being the neatest that can be given of the exercise, and the figures on pp. 109, 110 are a little wrong. Having had our grumble, we must now say that we think Mr. Constable has produced a very fair book, with neat solutions and good figures, but we do not consider such a book called for. Every mathematical master has such a collection either in manuscript or ready for *vivā voce* teaching, and has the more advanced works of McDowell and Casey on his shelves. We can, however, suggest that the little book may be of use in preparation for University Local, and other examinations, though we do not see its suitability for beginners.

Algebra. Part II. By E. J. Gross, M.A. Second Edition. (London : Rivington, 1882.)

WE are glad to see that this work has been so appreciated, that a second edition has been called for. The main defect of the first edition, in our opinion, was the plentiful crop of errata. This volume, we notice, has not been at all altered in the text, but very many of the errors have been corrected: we wish we could say that all errors had been removed, but this is not so. There are one or two curious slips: thus, for instance, in the Answers, p. 313, a correction is retained from the earlier edition, whilst the indicated alteration has been made in the text. Barring the errata, we again confidently commend Mr. Gross's book.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Pronunciation of Deaf Mutes who have been Taught to Speak

THE conclusions arrived at by M. Hément (*Comptes Rendus*, xciii. pp. 754, 861, 1095) and Mr. Axon (NATURE, vol. xxv. pp. 101, 409; *Comptes Rendus*, xciii. p. 904) concerning the influence of heredity upon the speech of the deaf cannot be accepted unless it can be shown that the peculiarities of utterance to which they have directed attention could not have arisen in the ordinary way by imitation of the speech of others.

Before we can decide whether any observed peculiarity in the utterance of a deaf person is due to inheritance or to imitation, we must know (1) at what age he became deaf; (2) whether the deafness was total or partial; and (3) whether, since the acquisition of speech by the sense of sight, the deaf subject has associated with persons who speak with the accent of his native district.

The remarks of M. Hément are valuable as the result of personal observations, but he has failed to be explicit upon these important points.

The cases referred to by Mr. Axon are equally inconclusive for the following reasons:—

1. In the first case noted (NATURE, vol. xxv. p. 101)—which is also the one to which Mr. Axon directs special attention in his recent letter to NATURE, vol. xxv. p. 409—we have a case of undoubtedly imitation through the sense of hearing.

Indeed it is stated in the *Phil. Trans.* No. 312, that some

weeks after recovery from an illness, this young man (Daniel Fraser) "began to hear and in process of time to understand speech. This naturally disposed him to imitate what he heard and to attempt to speak."

The account from which this is quoted, is evidently intended simply as a record of a case of recovery of hearing in a deaf mute, with subsequent acquisition of speech; and Mr. Axon himself admits that the writer mentions the inheritance of the Highland accent "in a purely incidental manner."

With due deference to Mr. Axon's opinion, it appears to me that this is not a case in point, and that it is not entitled to the same consideration as that of a person who, remaining deaf, acquired speech through the sense of sight, and has no opportunity of imitating by ear the pronunciation of others. It must also be remembered that this case is quoted from an old number of the *Phil. Trans.*, and cannot now be verified.

2. The circumstances quoted from the "Life and Journals of George Ticknor" (NATURE, vol. xxv. p. 101) are unreliable, because Ticknor assumed that all the pupils in the deaf and dumb schools he examined could never have heard a human sound,¹ whereas it is now known that a very large proportion of the deaf and dumb (probably more than 50 per cent.) could hear in infancy, and that of these a large proportion could also speak before becoming deaf.

3. In regard to the case of "E. R.," who had been taught articulation by Mr. Alley, of Manchester, Mr. Axon says, "that he became deaf and dumb at a very early age" (NATURE, vol. xxv. p. 191), but neglects to state at what age, which is very important—nor had Mr. Axon himself heard the articulation of this young man.

These are all the instances I know of, in which it is claimed that the pronunciation of any deaf person was due to inheritance, and I think I have shown that in all these cases the necessary data for such a conclusion have been wanting. I have already stated in a former communication (NATURE, vol. xxv. p. 124, *Comptes rendus*, xciii. p. 1036) that I have examined the pronunciation of at least 400 deaf children who have been taught to speak, without finding a single instance of peculiarity that could correctly be attributed to heredity. If any further argument is needed against inheritance of pronunciation, it is to be found in the universal fact that children who are born deaf are *always* also dumb.

That there is no incapacity of the vocal organs to account for this is evident, for these so-called "deaf-mutes" are now taught, through the medium of the sense of sight, to control the movements of their vocal organs so as to give utterance to intelligible words.

When we examine the languages and dialects of the world, I think we find that they have something in common, while each retain distinctive characteristics of its own. There seems to be a universal tendency to express the *emotions* in the same way. We speak, in fact, two languages at the same time:—One—the language of thought—arbitrary and conventional, acquired by imitation and not hereditary, consisting of articulations constituting words and sentences which can be recorded and preserved in written books; the other—the language of the emotions—natural and universal, consisting of looks and gestures, and of intonations of the voice. There seems good reason for the belief that this natural language of the emotions is instinctive, and therefore hereditary. From my own personal observations I feel sure that those who are interested in questions of heredity will find a rich field for inquiry in the study of the facial expressions and gestures of very young blind children; and in the natural sounds and the modulations of the voice of deaf infants.

Rome, March 6

ALEXANDER GRAHAM BELL

In his letter on the above subject in NATURE (vol. xxv. p. 409) Mr. Axon appears not to appreciate the value of negative testimony in scientific investigation.

The citation of cases in support of M. Hément's statement that deaf mutes who have been taught to speak do so with the accent of their native districts, obviously implies the promulgation of a theory that dialectal accent is due to physiological peculiarities (?) of the verbal organs), and that these are hereditary. This is shown by the objections raised to Prof. Graham Bell's statement that all such phenomena are due to "the unconscious recollection of former speech, and cannot be

¹ "Not one of the pupils of course can ever have heard a human sound," &c. "Life and Journals of George Ticknor," London, 1876. Vol. I. p. 196.

attributed to heredity." Now in order to be admissible, a theory must harmonise with all the phenomena. It is an admitted axiom in science that even one "negative" case is all but fatal to a theory. Dreams occasionally "come true," and persons who have been impressed by such coincidences base on them a belief in ghosts. But the great majority of dreams do not come true, and therefore science does not recognise the existence of ghosts. Careful investigation may bring out many explanations of a few cases of deaf "mutes" apparently speaking with the accent of their native districts, without attributing the phenomena to heredity. But if dialectal accent is hereditary, how are we to explain the 400 cases cited by Prof. Bell (to name one competent observer alone) in which no such accent was observable?

Mr. Axon quotes (apparently with approval) M. Hément's declaration of inability to conceive "how in losing the use of speech, deaf mutes should retain the unconscious memory of accent." I wish to suggest that such phenomena may be due to automatic activity of the cerebral tissue. In his last "Causerie," in the *Rappel*, M. Victor Meunier mentions the case of a young man, an inmate of a French asylum, who six years ago became deaf with the right ear through the effects of typhoid fever. He is occasionally conscious of sounds on the right, or deaf side, of which the left ear gives no indication whatever. He hears entire sentences, distinctly articulated, and as these are sometimes of an offensive character, they have involved him in many quarrels, as he has attributed them to perfectly innocent persons who have chanced to be near his right side at the time. Notwithstanding this hallucination, his judgment has remained sound, and having discovered that he sometimes hears (with his right ear) absent or stopped clocks strike loudly, he has learned to disregard any sounds but those which his left ear communicates to him. M. Luys, of the hospital of La Salpêtrière, gives many illustrations of such automatic activity.

Remembering that memory has a physiological basis, and believing in the psychical basis of language, I find it far less difficult to conceive that after the loss of speech deaf mutes should retain the "unconscious memory" of accent than that accent should be hereditary. Indeed the loss of speech might even be favourable to such retention; for the particular cells concerned might keep the original impression unimpaired by subsequent impressions, to be accurately given forth again when the requisite conditions came into operation.

This does not explain the case of Daniel Fraser, said to have been mute from his birth; but, on the other hand, "one swallow does not make a summer."

F. J. FARADAY

Manchester, March 4

Vignettes from Nature

WITH all due deference to Dr. Carpenter, for whose supreme authority on all matters of biological fact I have, of course, the profoundest respect, I must plead that he evidently has not looked into my little book, "Vignettes from Nature," but has taken his statements at second hand from the necessarily condensed account given in Mr. Wallace's kind review. Had he consulted the book itself, he would have found most of my remarks intentionally so guarded as to escape his strictures.

First, as to the sharks. Dr. Carpenter says, "None of these, in the judgment of Mr. Grant Allen and Mr. Wallace, surpassed the forty-feet sharks of the present time"; and he goes on to speak of a *Carcharodon* tooth from the Crag, 4 inches long by 3 broad. Now, in "Vignettes," p. 76, I wrote, "The teeth of what seems to have been the biggest known fish, a prodigious shark, are dredged up in the modern ooze of the Pacific. . . . They have become extinct at a very late date." I took my facts from Dr. Günther's "Study of Fishes," p. 321, where we read as follows:—"Carcharodon teeth are of very common occurrence in various tertiary strata. . . . Some individuals attained to an immense size, as we may judge from teeth found in the Crag, which are 4 inches wide at the base, and 5 inches long. . . . The naturalists of the *Challenger* expedition have made the highly interesting discovery that teeth of similar size are of common occurrence in the ooze of the Pacific, between Polynesia and the west coast of America. . . . The gigantic species to which these teeth belonged must have become extinct within a comparatively recent period." In short, the very shark which Dr. Carpenter claims as tertiary, I had previously claimed as also nearly modern.

Dr. Carpenter further says, "Is it clear that *Tridacna* is the largest known Mollusk? I should have thought it exceeded by

the gigantic *Ammonitidae*, &c." But if he will turn to "Vignettes," p. 75, he will see that I wrote, "No fossil bivalve molluscs to my knowledge are as big as . . . *tridacna*." The word I have italicised makes all the difference. On p. 77 Dr. Carpenter will see that allusion is made to the big Cephalopods, though perhaps none of these were very much larger than the largest modern gigantic squids.

As to the other points, they are really matters of language, and I will not take up your space by answering them in detail. When I spoke of "our whales," I certainly did not mean to exclude extinct whales: I merely meant to contrast them with the great secondary Saurians. Nor did I say that horses, elephants, &c., had been steadily increasing in size from "the earliest epoch of their appearance to the present day"; I said, "to the recent period," which is quite another thing. As I was writing for popular readers, not for biological critics like Dr. Carpenter, I felt bound to use the vague but comprehensible language of ordinary life; and so I described the mammoth as "recent," quite justifiably, I think, for my existing purpose. No technical words were used in the volume, and it was impossible always to find popular ones quite free from objection. But if Dr. Carpenter will kindly read the short chapter in question, I venture to think he will be willing to withdraw his present strictures. The object was merely to combat the vulgar notion that all the animals of all geological ages were positively gigantic; and in doing so, almost every animal mentioned by Dr. Carpenter was expressly adduced as an example.

In answer to ΦΠ, I should like to add that I used the word "accidental" in a strictly Pickwickian sense.

GRANT ALLEN

Miss Cobbe and Vivisection

WILL you allow me as one not only ardently interested in the pursuit of vivisection as a means of extending our knowledge, but also as a sincere hater of unnecessary cruelty to animals, to state the following facts which I know to be true:—

Some little time ago Miss Frances Power Cobbe, who has so identified herself with the cause of anti-vivisection, called on a distinguished man of science to endeavour, by persuasive speech and *vivā voce* argument, to gain him over to her cause. Three points were observable in Miss Cobbe's outward presentation, viz.: she had an ostrich feather in her bonnet; a bird of paradise in, on, or near her muff; and she carried an ivory-handled umbrella; consequently the distinguished man of science replied as follows:—

"Madam, charity begins at home; when you have given up wearing *ostrich feathers*, which are plucked from the *living bird*, causing the most exquisite pain, and birds of paradise, which, in order to enhance their beauty and lustre, are skinned *alive*; when you have abjured the use of *ivory*, because you know that the tusks are *cut out* of the dying elephant's jaw, then, and then only, come and upbraid me with the cruelty of my operations. The difference between us is, Madam, that *I* inflict pain in the pursuit of knowledge, and for the ultimate benefit of my fellow-creatures; *you* cause cruelty to be inflicted merely for your personal adornment"

H. H. JOHNSTON

Zoological Gardens, Tuesday

The Electrical Resistance of Carbon under Pressure

FROM the abstract of the proceedings of the Physical Society, given in NATURE, vol. xxv. p. 426, I learn that Prof. S. Thompson has been making some experiments which tend to show that the observed diminution of the resistance of carbon under pressure in such instruments as the carbon relay, rheostat, and microphone-transmitter is really due to the contact between the electrodes and the carbon. No doubt the greater portion of the observed diminution of resistance is due to this cause, and I have already pointed out in my paper on "the Influence of Stress and Strain on the Action of Physical Forces," Part ii., Electrical Conductivity, an abstract of which (NATURE, vol. xxv. p. 401) was read before the Royal Society on January 26, that the effect of a given amount of longitudinal traction or compression per unit area on the electrical resistance of some carbon rods was not greater than is the case with the metals tin and lead, for whereas a stress of 1 grammme per sq. cm. produced a variation of conductivity of from 7684×10^{-12} to 11420×10^{-12} per unit in the case of five carbon rods, the corresponding numbers were, with tin and lead, 10540×10^{-12} and 17310×10^{-12} respectively. The carbon rods were of the sort used for the purposes of electric